Does Choice Change Preferences?*

Carlos Alós-Ferrer[†]
University of Zurich

Georg D. Granic[‡]
Erasmus University Rotterdam

This Version: August 9th, 2018

Abstract

Do choices feed back into and alter preferences? Widespread evidence arising in psychology and neuroscience shows that preferences change in response to own choices, a phenomenon typically explained through cognitive dissonance. The evidence, however, presents serious shortcomings casting doubts on its relevance for economics. We present two experiments addressing these shortcomings. First, participants made standard decisions under risk rather than facing unfamiliar alternatives. Second, all choices were incentivized. Third, our novel experimental design avoids recently-exposed problems of experiments in psychology. The results show unsystematic effects which differ from and challenge conventional wisdom outside economics.

JEL Classification: C91 · D01 · D91

Keywords: Preference Change · Stability of Preferences · Construction of Preferences

*The authors gratefully acknowledge helpful comments from Anja Achtziger, Tanja Artiga Gonzalez, Aurelien Baillon, Christoph Engel, Drazen Prelec, and Peter Wakker. We also thank seminar participants at the Universities of Amsterdam, Cologne, Konstanz, Rotterdam, and Würzburg as well as the Max Planck Institute for Research on Collective Goods. Data was collected at the University of Cologne, which did not have an IRB at that point.

†Department of Economics, University of Zurich. Blümlisalpstrasse 10, 8006 Zurich, Switzerland. E-mail: carlos.alos-ferrer@econ.uzh.ch

‡Department of Applied Economics, Erasmus University. Erasmus School of Economics, PO Box 1739, 3000 DR Rotterdam (The Netherlands). E-mail: granic@ese.eur.nl

1 Introduction

Do we choose what we prefer, or do we prefer what we choose? The origins and stability of preferences are a topic of tantamount importance for economics (Stigler and Becker, 1977; Bowles, 1998; Fehr and Hoff, 2011). Positive economics is based on the possibility to organize observed choices through underlying preferences to forecast future choices, and normative economics rests on the assumption that welfare comparisons can be derived from those preferences. Yet, experimental evidence has repeatedly cast doubt on whether preferences can be seen as consistent and stable, or rather be swayed by extraneous elements. For example, Ariely et al. (2003) suggested that the formation of preferences might be affected by numerical anchoring, a conclusion challenged by Fudenberg et al. (2012) and Maniadis et al. (2014).

The most striking critique of the economists' approach to preferences is probably the claim that the mere act of choice can feed back into and alter pre-existing preferences, which has been long considered as a firmly-established fact in psychology, neuroscience, and cognitive science. Widely-replicated empirical evidence going back to Brehm (1956) (see, e.g., Harmon-Jones and Mills, 1999; Ariely and Norton, 2008) shows specific patterns of preference change, elicited by liking ratings or rankings, after a binary choice and without any additional information being received. A classical explanation involves cognitive dissonance theory (Festinger, 1957; Joule, 1986), which in this context essentially states that every choice involves facing tradeoffs, and that the negative aspects of the chosen option and the positive aspects of the rejected one create a psychological tension which the decision maker attempts to reduce by changing the preferences. More generally, cognitive dissonance theory asserts that attitudes and beliefs are often brought in line with actions (as in the induced-compliance paradigm; Festinger and Carlsmith, 1959), especially if they are costly or effortful (as in the effort-justification paradigm; Aronson and Mills, 1959). The importance of these phenomena for economics, especially with respect to beliefs, has been previously pointed out by Akerlof and Dickens (1982), Konow (2000), and Oxoby (2004).

However, in spite of an apparently-overwhelming body of evidence, the phenomenon of *choice-induced preference change* has so far been met with skepticism in economics. There are three good reasons for this reaction. First, most of the experiments which the psychological evidence builds upon test preferences over unfamiliar or ill-defined choices, as e.g. liking ratings of artistic paintings or hypothetical holiday destinations described exclusively by the destination name. In such cases, preferences might not be fully formed and behavior might be extremely noisy, with measurements reflecting preference formation and not change.[1] Second, experimental choices in this area are typically hypothetical and unincentivized, casting doubts on whether observed behavior

[1] One of the few examples where evidence for choice-induced preference change has been found outside the lab is the study of Mullainathan and Washington (2009), which exploited turnout constraints due to the voting age restriction to examine the effect of voting in subsequent political preferences in the United States. However, a similar study by Elinder (2012) found no evidence of preference change.

is actually indicative of preferences in economic settings. Third, it has been recently shown that the experimental paradigm introduced by Brehm (1956), which has guided the development of the literature for over 50 years, is regrettably flawed, in the sense that it contains a statistical bias that can result in apparent preference change even if participants have immutable preferences, as long as they display noisy (but unbiased) behavior (Chen and Risen, 2010; Izuma and Murayama, 2013; Alós-Ferrer and Shi, 2015). Although this flaw does not affect other aspects of cognitive dissonance and the specific phenomenon has been reexamined with improved designs (Sharot et al., 2010; Alós-Ferrer et al., 2012), the fact remains that the body of evidence in favor of choice-induced preference change cannot be currently called "overwhelming" anymore.

In this work, we ask the question of whether the phenomenon of preference change due to own choice actually occurs in economic settings. We provide a novel experimental paradigm addressing all three critiques mentioned above. Participants face fully-incentivized choices within a standard economic task (decisions under risk). Further, the very fact that we use a well-defined economic domain allows us to develop a design which does not suffer from statistical biases. We then report on two separate experiments (for robustness reasons) relying on the same basic design.

The results are striking. While we do find evidence for choice-induced preference change, the characteristics of the change itself are not stable across experiments and might depend on apparently irrelevant details. The implication is that either the phenomenon is real but not systematic (or not of a sufficient magnitude to be easily measurable) or none of the currently available theories from psychology, and in particular cognitive dissonance theory, can actually explain the phenomenon.

The paper is structured as follows. Section 2 briefly reviews the typical paradigms and design difficulties appearing in psychology and presents our improved paradigm. Section 3 presents the specific design and describes the experiments. Section 4 presents the results, and Section 5 concludes.

2 Previous Evidence and an Improved, Economic Approach

Since Brehm (1956), which motivated the development of cognitive dissonance theory (Festinger, 1957), most evidence on choice-induced preference change has been collected using the *free choice paradigm* (FCP). Experiments following this approach collect data on a finite set of alternatives, A, in three stages. In Stage 1 (the first evaluation phase), rankings or ratings on A are elicited. For concreteness, we focus on rankings. Let $R_1(a)$ denote the numerical rank of each alternative $a \in A$, with lower numbers indicating a better placement (first, second, etc). In Stage 2 (the choice phase), the experimenter (or a computer algorithm) produces choice pairs $(a, b) \in A^2$, $a \neq b$, such that $R_1(a)$ and $R_1(b)$ were close (e.g., differing by 1 rank), and the participant is asked to choose an option out of each constructed pair. In Stage 3 (the second evaluation phase), new rankings $R_2(a)$ are elicited for each $a \in A$.

For a pair (a, b) offered in the choice phase, suppose a was the alternative chosen by the participant, and b the one rejected. The dependent variable in all experiments using the FCP is the *spread*

$$S(a, b) = (R_2(a) - R_2(b)) - (R_1(a) - R_1(b)) \tag{1}$$

The hypothesis, which has been confirmed in a large number of experiments, is that $E[S(a, b)] < 0$ ($E[S(a, b)] > 0$ for the case of ratings), where $E[\cdot]$ denotes the expectation. This is interpreted as evidence that chosen options are typically reevaluated upwards and rejected ones are reevaluated downwards, the argument being that, with immutable preferences, one should have observed no change in ranks.

This design contains a methodological flaw, as was first pointed out by Chen and Risen (2010) and further discussed in Izuma and Murayama (2013) and Alós-Ferrer and Shi (2015). Although the point is subtle (and hence escaped notice for over 50 years), essentially the problem is that in the construction of $S(a, b)$, the identities of a and b are not randomized. In a stochastic choice setting, where both choices and rankings follow from underlying preferences with zero-mean noise, the fact that a has been chosen over b carries information on the underlying preferences. Roughly speaking, noisy rankings in Phase 1 will tend to be corrected in the direction of the true preference in Phase 3, and conditioning on choice (which reveals information on the preference) creates a purely statistical bias.[2] This has motivated improved designs which aim to randomize which option is chosen. In the *blind-choice paradigm* (Sharot et al., 2010), participants are deceived to make them believe they have chosen whatever option the experimenter has predetermined. In the *implicit-choice paradigm* (Alós-Ferrer et al., 2012), "chosen" options are randomized (say, a) and the participant is offered the choice of a and a third option with a low elicited evaluation, and reciprocally for the "rejected" option, resulting in choices typically (but not always) aligned with the predetermined ones. However, both paradigms retained non-incentivized choices and unfamiliar alternatives as described above.

Our design keeps the basic three-phase structure typical of the previous literature (hence allowing for easy comparison) but improves upon it in three respects. First, all three phases are incentivized following standard practices and methods in economics (details are provided below). Second, instead of hypothetical holiday destinations or unfamiliar artistic paintings, the objects of choice are standard lotteries. That is, we conduct our experiments within the well-studied domain of decisions under risk. It is

[2]The issue is a relatively complex one. Chen and Risen (2010) provided a formal model and claimed that, under a certain set of assumptions on the process generating elicited evaluations and choices, $E[S(a, b)] > 0$ in the FCP (for ratings). Their proof was shown to be itself flawed by Alós-Ferrer and Shi (2015), who in turn showed that $E[S(a, b)] \neq 0$ in general, might be either positive or negative, but $E[S(a, b)] > 0$ will hold if $R_1(a) = R_1(b)$ (as implemented in many experiments). Both Izuma and Murayama (2013) and Alós-Ferrer and Shi (2015) provided computer simulations of artificial agents with immutable preferences and zero-mean noise in evaluations and choices, and found $E[S(a, b)] \neq 0$ when such agents "participate" in an FCP experiment.

precisely these two changes which allow us to completely bypass the third difficulty and construct a design free of statistical biases. The reason is that, within a well-defined domain as that of lotteries, and unlike with the fixed sets of options used in typical psychology experiments, we can construct new lotteries on the fly in the choice phase (that is, not part of the originally ranked sets). Specifically, the essence of our design is as follows: given any pair (a, b), where a has been randomly determined (by the experimenter) to be "chosen" and b to be "rejected," we construct new lotteries c and d with the intention of offering the participant the actual choice pairs (a, c) and (b, d). To ensure that the participant actually chooses a over c (making a chosen) and d over b (making b rejected), we rely on first-order stochastic dominance (FOSD). Specifically, we construct d as a lottery which first-order stochastically dominates b, and c as a lottery first-order stochastically dominated by a. If a lottery stochastically dominates another lottery the former should be ranked above the latter, independently of underlying risk attitudes, as long as participants prefer larger amounts of money over smaller ones.[3] Hence one would expect overwhelming compliance with the randomly determined choices.

To derive our experimental hypotheses, assume that the values $R_i(a)$ follow from a value-generating process which (noisily) reflects preferences and that choice among lotteries obeys FOSD. By relying on FOSD, we will randomly and exogenously assign lotteries to being chosen or not, hence manipulating decision makers into choosing them or not. If choice-induced preference change is a real phenomenon, lotteries flagged as chosen should be reevaluated upwards (resulting in lower ranks) compared to lotteries flagged as rejected. Rather than translating this into a hypothesis on the spread as given in (1), we note that in our framework it is possible to give a sharper implication which is transparently free of any possible statistical bias. Suppose the decision maker has been given separate sets of options to rank, A and A' (containing the same number of options), and we focus on options ranked identically within their respective sets, $R_1(x|A) = R_1(x'|A')$. Now suppose we assign x to be chosen and x' to be rejected. The hypothesis above translates into $R_1(x|A) - R_2(x|A) > R_1(x'|A') - R_2(x'|A')$ (recall that lower ranks indicate a stronger preference), or, since, $R_1(x|A) = R_1(x'|A')$, simply $R_2(x|A) < R_2(x'|A')$ (in expected terms).

Let x denote an alternative within a set A. From the *ex ante* point of view, the reasoning above implies, for any fixed rank r,

$$E[R_2(x)|R_1(x) = r, x \text{ chosen}] < E[R_2(x)|R_1(x) = r, x \text{ rejected}] \tag{2}$$

which delivers our experimental hypothesis for any alternative part of the choice manipulation. Note that, contrary to hypotheses based on (1) as used in psychology, inequality

[3] This follows immediately under Expected Utility Theory, but it is also a straightforward implication of other theories as e.g. Cumulative Prospect Theory. In their first version of prospect theory, Kahneman and Tversky (1979) assumed that transparently dominated lotteries were eliminated in the "editing phase."

Figure 1: Schematic overview experimental design.

Stage 1, ranking R_1: rank 6 lotteries in each of 6 lottery groups **Identify** $a = 3^{rd}$-ranked and $b = 4^{th}$-ranked in each group

33%	33%	33%
2 FCP-lottery groups	2 Target lottery groups	2 Target lottery groups
↓	↓	↓
FCP-pair (a, b)	target pair congruent (a, c), $a \succ_{FOSD} c$ (b, d), $d \succ_{FOSD} b$	target pair incongruent (a, c), $c \succ_{FOSD} a$ (b, d), $b \succ_{FOSD} d$

Stage 2, choice task: Independent choices presented.		
(a, b)	(a, c) (b, d)	(a, c) (b, d)

Stage 3, ranking R_2: re-rank lotteries

(2) allows separate tests for the two alternatives involved in a given pair (a, b) manipulated as described above. This is also natural in our design, since there is no direct choice among a and b.

3 Experimental Design and Procedures

We conducted two incentivized laboratory experiments to investigate the economic validity of choice-induced preference change. In each experiment, participants were asked to rank and choose among simple lotteries (two outcomes, two probabilities), as frequently employed in judgment and decision making (e.g. Lichtenstein and Slovic, 1971; Grether and Plott, 1979). To obtain transparent FOSD relationships, we manipulated one lottery attribute keeping the other one constant. Experiment 1 manipulated the probability domain and Experiment 2 manipulated the outcome domain. Otherwise, both experiments followed the same experimental design, described below and summarized in Figure 1.

3.1 Basic design, incentives, and identification

There were three incentivized decision stages: Two ranking tasks in stages 1 (R_1) and 3 (R_2), and a choice task in Stage 2. Task-related payments were deferred to the end of the experiments, i.e. after all relevant decisions were made. We created two lists of 36 lotteries, one for each experiment. All outcomes were positive EUR amounts and lotteries were presented as pie charts augmented by a numerical representation of possible outcomes and their corresponding probabilities. The order of presentation and the positioning of lotteries on-screen was randomized. No lottery was used twice.[4]

Ranking tasks R_1 and R_2. The 36 lotteries were divided randomly at the participant-level into six different groups containing six lotteries each. The composition of the six groups remained fixed during an experiment. In the first (ranking) stage R_1, participants were presented with each group of lotteries sequentially and instructed to rank lotteries from most (rank 1) to least preferred (rank 6). The third-stage ranking task R_2 was identical to R_1 except for a randomized presentation order. The instructions for R_2 explicitly stated that the task was not a memory test.

We adapted the *ordinal payoff scheme* to incentivize rankings (see e.g. Tversky et al., 1990; Cubitt et al., 2004; Alós-Ferrer et al., 2016). For each ranking task, the computer randomly selected one of the six groups and two lotteries within it. From those two, the lottery that the participant had ranked as more desirable was selected and played out. This payment mechanism elicits ordinal preferences in an incentive-compatible way (under the reduction of compound lotteries axiom).

Choice task. The choice task in stage 2 consisted of 10 choices. Each of them asked participants to choose one of two lotteries. We used the 3rd- and 4th-ranked lotteries (henceforth a and b, respectively) according to R_1-rankings from the same lottery group to construct choice pairs. The middle ranks were chosen to avoid floor and ceiling effects in potential preference ramifications following choice. For two of the six lottery groups we asked participants to choose among (a, b) directly. This design replicates the classical (but flawed) free-choice-paradigm (FCP) extensively used in psychology (Brehm, 1956).[5] We refer to those as *FCP-pairs*. For the remaining four lottery groups the construction of choice pairs followed the design already sketched above (Figure 1), which relies on comparing the target lotteries with newly-generated ones. In these groups, lotteries a and b were part of two independent choice pairs (a, c) and (b, d). We will refer to those as *target pairs*.

The key feature of the protocol is to assign what is chosen and rejected randomly. In two of the four remaining lottery groups, lottery c dominated lottery a and lottery b dominated lottery d by FOSD, creating four target pairs. Participants who obeyed FOSD thus 'rejected' a and 'chose' b. We refer to these pairs as *incongruent*, as induced choice patterns were opposite of participants' revealed R_1-rankings for a and b. In the remaining two lottery groups, lottery a dominated lottery c and lottery d dominated lottery b, creating four further target pairs. We call these pairs *congruent*, as choice patterns reinforced previously stated R_1-rankings. Lottery groups from the ranking stages were randomly assigned to the different construction conditions at the participant

[4]The different FOSD-construction processes between experiments made it necessary to create experiment-specific sets of lotteries. Our guiding principles in creating lotteries as well as the lotteries themselves and the experimental instructions can be found in the supplementary materials.

[5]To the best of our knowledge, we are the first to study choice-induced preference change in the domain of simple lotteries. We deemed a conceptual replication necessary to establish comparability with the existing literature.

level. Each participant faced two FCP-pairs, four incongruent target pairs, and four congruent target pairs.

After the experiment, a participant's payment for the choice task was derived by selecting one of the 10 lottery pairs at random. The participant then received the lottery she had chosen and that lottery was played out.

Identification. Our design steers free of the statistical biases identified in previous designs and allows us to isolate the pure effect of choice on preference. Lotteries c and d were solely constructed for the purpose of inducing the intended choice patterns. That is, we orthogonalized choice and preference information. Choices and their implied 'chosen' and 'rejected' labels were uninformative about underlying lottery valuations. Assuming that participants obeyed FOSD, these labels were purely random. Lotteries a were chosen in congruent pairs and rejected in incongruent ones (vice versa for lotteries b). Measuring choice-induced preference change, hence, reduced to a comparison of post-choice R_2-ranks between congruent and incongruent conditions for lotteries a and b separately.

Identification of choice-induced preference change in this design hinges on participants obeying FOSD. In our experiments, we introduced transparent FOSD-relationships and employed treatment manipulations to make sure that the FOSD-compliance rate was stable. We will present additional results demonstrating that violations of FOSD were empirically rare events which did not undermine the validity of the choice-randomization procedure.

3.2 Procedures

The experiments were carried out at the Cologne Laboratory for Economic Research and participants were recruited from the same standard student pool using the software ORSEE (Greiner, 2015). All tasks were computer-implemented using the software z-Tree (Fischbacher, 2007). In total, we recruited 296 university students, 140 for Experiment 1 (80 females, mean age 23.3) and 156 for Experiment 2 (91 females, mean age 23.7). Sessions lasted about 1h 20 min on average, with participants' average remuneration amounting to €15.77 and €16.22, respectively.

In Experiment 1, we manipulated the probability domain keeping constant outcomes to obtain FOSD-relationships in choice pairs. That is, a lottery intended to be chosen (rejected) was paired with a new, unique lottery with a lower (higher) probability to win the high amount and a higher (lower) probability to win the low amount. In Experiment 2, FOSD-relationships were obtained via manipulations of the high amount to win in the outcome domain keeping constant probabilities. Lotteries intended to be chosen (rejected) were paired with new, unique lotteries with decreased (increased) high-amounts to win.

To further ensure that FOSD induced behavior as expected independently of other factors, in each experiment participants were randomly assigned to one of two possible treatments. Those treatments targeted the FOSD compliance rate by increasing the transparency in FOSD relationships and the costs of FOSD violations. In the *High* treatment, FOSD relationships were established by adding or subtracting four percentage points in probabilities for Experiment 1 and by adding or subtracting 20 Eurocents to the high amounts to win in Experiment 2. In the *Low* treatment, probabilities were changed by two percentage points and outcomes by eight Eurocents. Each participant faced two FCP pairs, four incongruent target pairs, and four congruent target pairs.[6] If behavior follows FOSD independently of the level of incentives, there should be no differences in the rate of FOSD violations across treatments.

4 Results

Since our manipulation rests on decisions respecting FOSD, we first show that violations of FOSD were rare events which occurred unsystematically. We then present our main results concerning target pairs. Last, for comparability with the previous literature, we establish that our FCP sub-design replicates the standard pattern observed in the existing literature (even though the design flaw in the FCP prevents drawing conclusions from this fact).

4.1 FOSD violations

Violations of FOSD were very rare events and did not undermine the validity of the choice randomization procedure. In total we observed 2,368 decisions where one alternative dominated the other in the FOSD sense, 1,120 in Experiment 1 and 1,248 in Experiment 2. Only a small fraction of these decisions violated FOSD, respectively 24 (2.1%) and 31 (2.5%). There were no systematic differences in FOSD violations between experimental treatments nor between congruent and incongruent choice-pairs.[7] Hence, we will pool the High/Low treatments for further analysis.

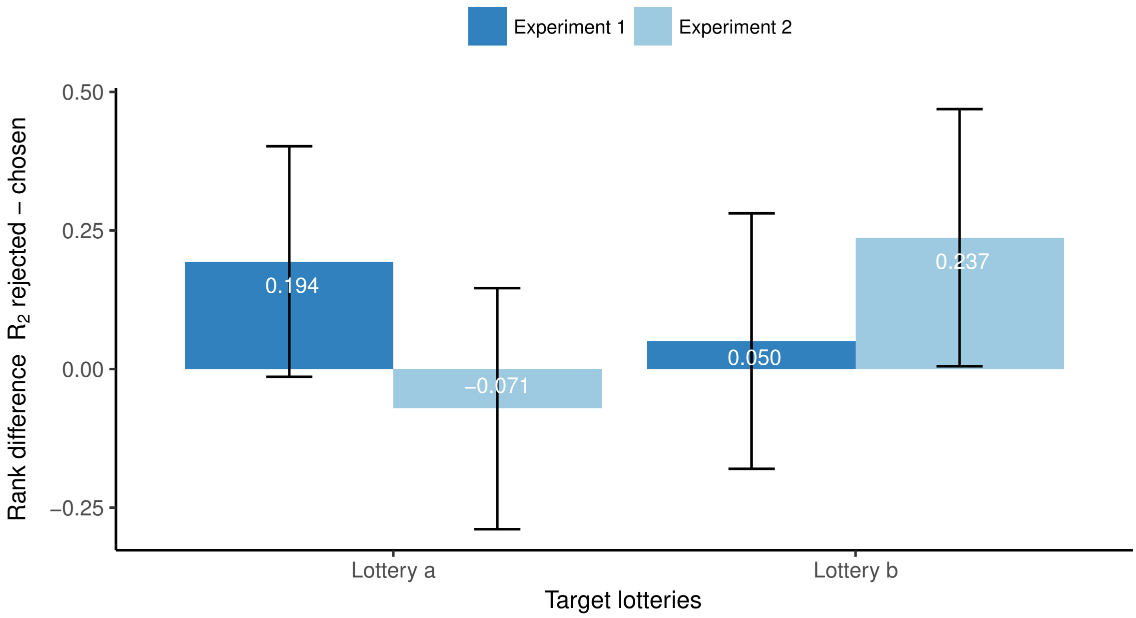
4.2 Target pairs

Under choice-induced preference change, we expected a positive R_2-rank difference between rejected and chosen lotteries of the same R_1-rank (since higher ranks signify lower preference). Figure 2 shows the corresponding mean participant-averaged R_2-rank difference for lotteries a and b after eliminating the rare observations part of FOSD violations.

[6]Let $(p : x;\ y)$ denote a lottery that pays €x with probability p and €y with the complementary probability $1 - p$. Let, for example, $a = (0.25 : 12;\ 2)$ and $b = (0.20 : 10;\ 5)$ and suppose we wish to construct congruent target pairs (a, c) and (b, d), i.e. we want a to be chosen in the pair (a, c) and b to be rejected in the pair (b, d). In the High treatment of Experiment 1, the construction yields $c = (0.21 : 12;\ 2)$ and $d = (0.24 : 10;\ 5)$. In the High treatment of Experiment 2, we obtain $c = (0.25 : 11.8;\ 2)$ and $d = (0.20 : 10.2;\ 5)$.

[7]In Experiment 1 we observed 15 out of 24 FOSD violations in the High treatment and 13 out of 24 violations in congruent pairs. In Experiment 2, the corresponding figures are 17 out of 31 and 20 out of 31.

Figure 2: Mean and 95% CI of participant-averaged R_2-rank difference between rejected and chosen flag-conditions in target pairs, Experiment 1 and Experiment 2.



We observed a two-fold pattern of asymmetries in R_2-rank differences, which was unexpected from the point of view of the existing (psychological) literature.

Uninformative choices increased rank evaluations for a-type lotteries (i.e., 3rd-ranked in R_1) in Experiment 1 and b-type lotteries in Experiment 2 (i.e., 4th-ranked in R_1). The corresponding chosen a and b lotteries were R_2-ranked, on average, 0.194 and 0.237 positions better than comparable but rejected lotteries a and b, respectively. According to two-sided Wilcoxon-signed-rank tests, the corresponding median participant-averaged R_2-rank differences were significantly different from zero ($p = 0.028$ in Experiment 1, $p = 0.023$ in Experiment 2).[8] This evidence is in line with choice-induced preference change.

However, for type-b lotteries in Experiment 1 and type-a ones in Experiment 2, we observed no effect of choice on preference. The mean R_2-rank difference between rejected and chosen lotteries were -0.071 and 0.050, respectively. Wilcoxon-signed-rank tests failed to detect any significant difference in R_2-ranks here ($p = 0.827$ in Experiment 1, $p = 0.410$ in Experiment 2).

Our main observations were confirmed by panel generalized-least-squares regressions with participant random-effects on stated R_2-ranks. Table 1 presents the corresponding results. All standard errors were clustered at the participant level and we used the full sample of observations (not excluding FOSD violations). Independent observations were taken at the participant-lottery level. The *Choice* dummy, taking the value 1 if a lottery was chosen, was negative and significant at the 5% level for type-a lotteries in

[8]The significance of all reported hypothesis tests in this paper is robust with respect to Holm-Bonferroni corrections to account for multiple-hypotheses testing.

Table 1: Panel generalized least squares regressions on R_2-ranks with participant random-effects. Standard errors, reported in parentheses, are clustered at the participant level. *High*: treatment dummy. *Dominance*: dummy, 1 if decision in the choice task violated FOSD. Demographic controls: age, gender, previous participation in a statistics course, and following an economics study program. Lottery controls: probabilities, outcomes and expected values of choice pair lotteries. Presentation controls: on-screen position of lotteries, and choice period.

Dependent variable	Stated R_2-rank in all regressions			
Sample	Experiment 1 Lottery		Experiment 2 Lottery	
	a, $R_1 = 3$	b, $R_1 = 4$	a, $R_1 = 3$	b, $R_1 = 4$
Choice	-0.194	-0.064	0.067	-0.245
	(0.098)	(0.113)	(0.104)	(0.110)
High	-0.005	-0.197	-0.045	0.421
	(0.169)	(0.175)	(0.253)	(0.254)
Dominance	-0.691	-0.417	-0.243	0.066
	(0.477)	(0.454)	(0.296)	(0.357)
Constant	4.190	3.476	-0.202	1.228
	(0.602)	(0.522)	(0.873)	(0.819)
Number of participants	140	140	156	156
Number of observations	560	560	624	624
Demographic controls	Yes	Yes	Yes	Yes
Lottery controls	Yes	Yes	Yes	Yes
Presentation controls	Yes	Yes	Yes	Yes
Cluster robust std. err.	Yes	Yes	Yes	Yes

Experiment 1 and type-b lotteries in Experiment 2; in contrast, the coefficients for type-b lotteries in Experiment 1 and type-a lotteries in Experiment 2 were insignificant. The results are robust to the inclusion of a broad set of control variables which we discuss more thoroughly in the online supplementary materials.[9]

4.3 FCP pairs

For FCP pairs, the results replicated the standard choice-induced preference change pattern. Lotteries chosen in FCP pairs improved on average by 0.279 ranks and 0.391 ranks from R_1 to R_2 in Experiments 1 and 2, respectively. Two-sided Wilcoxon-signed-rank tests corroborate that the medians of these participant-averaged rank differences were significantly different from zero ($p = 0.001$ and $p < 0.001$, respectively). Before the

[9] We also ran multiple robustness checks and alternative analyses, all of which confirmed our findings. Specifically, we ran parametric t-tests, panel generalized-least-squares regressions on R_2-ranks with participant fixed-effects, and panel-tobit regressions on R_2-ranks, acknowledging that ranks are censored from below and from above. In addition, all regression analyses were run both including and excluding observations involved in FOSD violations.

critiques on the FCP were raised (Chen and Risen, 2010; Izuma and Murayama, 2013; Alós-Ferrer and Shi, 2015), this could have been interpreted as evidence of preference change. However, since the "chosen" options were not randomly predetermined, the results merely reflect a statistical bias because being "chosen" carries information on the underlying preferences whose change one intends to measure. The problem is particularly acute if choices are more indicative of preferences than elicited rankings, as has been argued in the economics literature (Schmidt and Hey, 2004; Butler and Loomes, 2007). This might be the case in our data: pre-choice R_1-ranks were only weakly indicative of choices made in the 280 and 312 distinct FCP-pairs in our experiments. Participants chose lottery a, the lottery revealed to be preferred in R_1, in only 56% and 58% of these pairs, respectively. These results demonstrate that the design flaw associated with the FCP setup is a concern for economically valid domains and highlight the need for improved designs (as the one generating our target pairs) to measure choice-induced preference change.

5 Discussion

Undoubtedly, psychological insights have had a profound impact in modern economics. However, it has to be recognized that economics and psychology have overlapping but different objects of study. Psychologists have studied *attitude* change for decades, but have not been concerned with *economic* choices *per se*. The latter require well-defined consequences and incentives. When a psychologist studies whether our liking of an artistic painting that we had never seen before or our abstract predisposition towards spending holidays in a certain country (without reference to costs or duration) change after we make the mental experiment of "putting us there" (making a hypothetical choice), he or she is studying a perfectly valid psychological construct. An economist, however, is fully justified to ask whether what we are seeing is initial taste formation, the explicit discovery of our actual tastes, or an actual change of a well-defined preference.

The differences are consequential. With respect to preferences, economics is mostly concerned with choices among well-defined alternatives with well-understood consequences, or at least well-defined beliefs over those. Are preferences, *as understood by economists and as relevant for economics*, affected by choice-induced preference change? Answering this question requires a design with explicitly economic, well-understood choices which are fully incentivized in a transparent way. Additionally, it has been shown that experimental designs by psychologists suffer from statistical biases, which are partly due to the fact that those designs operate on domains with hypothetical and ill-defined choices. In this work, we present a design which addresses all those difficulties. Choices are explicitly economic in nature, well-defined, with clearly-understood consequences, and fully incentivized. Using this domain allows us to avoid any statistical biases in the design by effectively randomizing choice through the recourse to first-order stochastic dominance.

We conducted two experiments with this design, with separate samples and different manipulations leading to FOSD relations. We do find evidence which could be interpreted as choice-induced preference change. However, it is inconsistent in the sense that the effect is observed for some lotteries and not others in one experiment, and exactly the opposite pattern in the other experiment. This two-fold, asymmetric pattern is in stark contrast with the extensive experimental literature from psychology which argues for a robust and universally-reproducible choice-effect on preferences. Furthermore, our observations are difficult to reconcile with any of the existing theoretical accounts of the phenomenon, of which cognitive dissonance theory is the most prominent example. To the best of our knowledge, neither this theory nor any of the alternatives put forward over the years (e.g. Bem, 1967a,b) would predict the asymmetries we observe.

Of course, the asymmetries rest on lack of significance, and absence of evidence is not evidence of absence, but the simplest explanation for the observed pattern at this point is that choice-induced preference change in economic domains exists but is not as strong as has been previously implied in other disciplines, and might depend on seemingly-unimportant factors.

We understand our results as an opportunity to stimulate further research into this topic. Our data does reflect an (inconsistent) effect of choice on preferences. Future research should further investigate preference-change patterns to gain a better understanding of the boundary conditions of the phenomenon. For example, we study the pure effect of uninformative choice on preference, whereas most existing previous studies in psychology, which regrettably used a flawed design, can be seen as incorporating some form of trade-off in choice. If trade-offs are a necessary precondition for the phenomenon to emerge then appropriate experimental designs will have to be developed, with an eye on separating this potential source from the pure effect of choice.

At this point, we conclude that economics should keep an open mind on the phenomenon of choice-induced preference change, but maintain a healthy skepticism with regards to its implications for economics. Down the road, it is clear that understanding phenomena such as decision makers' need for logical and cognitive consistency (e.g., with their own past choices) will help develop better preference elicitation methods and improve predictive accuracy. At this point, however, there is no consistent theoretical understanding of those phenomena and no clear way to import stylized facts from other disciplines into economics.

References

Akerlof, G. A. and Dickens, W. T. (1982). The Economic Consequences of Cognitive Dissonance. *American Economic Review*, 72:307–319.

Alós-Ferrer, C., Granić, D.-G., Kern, J., and Wagner, A. K. (2016). Preference Reversals: Time and Again. *Journal of Risk and Uncertainty*, 52(1):65–97.

Alós-Ferrer, C., Granić, D.-G., Shi, F., and Wagner, A. K. (2012). Choices and Preferences: Evidence from Implicit Choices and Response Times. *Journal of Experimental Social Psychology*, 48(6):1336–1342.

Alós-Ferrer, C. and Shi, F. (2015). Choice-Induced Preference Change and the Free-Choice Paradigm: A Clarification. *Judgment and Decision Making*, 10(1):34–49.

Ariely, D., Loewenstein, G., and Prelec, D. (2003). "Coherent Arbitrariness": Stable Demand Curves without Stable Preferences. *Quarterly Journal of Economics*, 118(1):73–105.

Ariely, D. and Norton, M. I. (2008). How Actions Create – Not Just Reveal – Preferences. *Trends in Cognitive Sciences*, 12(1):13–16.

Aronson, E. and Mills, J. (1959). The Effect of Severity of Initiation on Liking for a Group. *The Journal of Abnormal and Social Psychology*, 59(2):177–181.

Bem, D. J. (1967a). Self-Perception: An Alternative Interpretation of Cognitive Dissonance Phenomena. *Psychological Review*, 74(3):183–200.

Bem, D. J. (1967b). Self-Perception: The Dependent Variable of Human Performance. *Organizational Behavior and Human Performance*, 2(2):105–121.

Bowles, S. (1998). Endogenous Preferences: The Cultural Consequences of Markets and Other Economic Institutions. *Journal of Economic Literature*, 36(1):75–111.

Brehm, J. W. (1956). Postdecision Changes in the Desirability of Alternatives. *The Journal of Abnormal and Social Psychology*, 52(3):384–389.

Butler, D. J. and Loomes, G. (2007). Imprecision as an Account of the Preference Reversal Phenomenon. *American Economic Review*, 97(1):277–297.

Chen, M. K. and Risen, J. L. (2010). How Choice Affects and Reflects Preferences: Revisiting the Free-Choice Paradigm. *Journal of Personality and Social Psychology*, 99(4):573–594.

Cubitt, R. P., Munro, A., and Starmer, C. (2004). Testing Explanations of Preference Reversal. *Economic Journal*, 114(497):709–726.

Elinder, M. (2012). Correcting Mistakes: Cognitive Dissonance and Political Attitudes in Sweden and the United States. *Public Choice*, 153:235–249.

Fehr, E. and Hoff, K. (2011). Tastes, Castes and Culture: The Influence of Society on Preferences. *Economic Journal*, 121(556):F396–F412.

Festinger, L. (1957). *A Theory of Cognitive Dissonance*. Stanford University Press, Stanford, CA.

Festinger, L. and Carlsmith, J. M. (1959). Cognitive Consequences of Forced Compliance. *The Journal of Abnormal and Social Psychology*, 58(2):203–210.

Fischbacher, U. (2007). z-Tree: Zurich Toolbox for Ready-Made Economic Experiments. *Experimental Economics*, 10(2):171–178.

Fudenberg, D., Levine, D. K., and Maniadis, Z. (2012). On the Robustness of Anchoring Effects in WTP and WTA Experiments. *American Economic Journal: Microeconomics*, 4(2):131–145.

Greiner, B. (2015). Subject Pool Recruitment Procedures: Organizing Experiments with ORSEE. *Journal of the Economic Science Association*, 1:114–125.

Grether, D. M. and Plott, C. R. (1979). Theory of Choice and the Preference Reversal Phenomenon. *American Economic Review*, 69(4):623–638.

Harmon-Jones, E. and Mills, J. E. (1999). *Cognitive Dissonance: Progress on a Pivotal Theory in Social Psychology*. American Psychological Association, Washington, DC.

Izuma, K. and Murayama, K. (2013). Choice-Induced Preference Change in the Free-Choice Paradigm: A Critical Methodological Review. *Frontiers in Psychology*, 4(Article 41):1–12.

Joule, R. V. (1986). Twenty Five On: Yet Another Version of Cognitive Dissonance Theory? *European Journal of Social Psychology*, 16:65–78.

Kahneman, D. and Tversky, A. (1979). Prospect Theory: An Analysis of Decision Under Risk. *Econometrica*, 47(2):263–291.

Konow, J. (2000). Fair Shares: Accountability and Cognitive Dissonance in Allocation Decisions. *American Economic Review*, 90(4):1072–1091.

Lichtenstein, S. and Slovic, P. (1971). Reversals of Preference Between Bids and Choices in Gambling Decisions. *Journal of Experimental Psychology*, 89(1):46–55.

Maniadis, Z., Tufano, F., and List, J. A. (2014). One Swallow Doesn't Make a Summer: New Evidence on Anchoring Effects. *American Economic Review*, 104(1):277–290.

Mullainathan, S. and Washington, E. (2009). Sticking with Your Vote: Cognitive Dissonance and Political Attitudes. *American Economic Journal: Applied Economics*, 1(1):86–111.

Oxoby, R. J. (2004). Cognitive Dissonance, Status and Growth of the Underclass. *Economic Journal*, 114(498):727–749.

Schmidt, U. and Hey, J. D. (2004). Are Preference Reversals Errors? An Experimental Investigation. *Journal of Risk and Uncertainty*, 29(3):207–218.

Sharot, T., Velasquez, C. M., and Dolan, R. J. (2010). Do Decisions Shape Preference? Evidence from Blind Choice. *Psychological Science*, 21(9):1231–1235.

Stigler, G. J. and Becker, G. S. (1977). De Gustibus Non Est Disputandum. *American Economic Review*, 67(2):76–90.

Tversky, A., Slovic, P., and Kahneman, D. (1990). The Causes of Preference Reversal. *American Economic Review*, 80(1):204–217.

Online Appendix

A Robustness analyses

A.1 Experiment 1

We ran generalized least squares regressions with participant fixed-effects and participant random-effects separately on stated R_2-ranks as the dependent variable. Table 2 presents the results. All standard errors were clustered at the participant level and we used the full sample of observations. Independent observations were taken at the participant-lottery level. Our regression analysis confirms the findings reported in the main text. The *Choice* dummy, taking value 1 if a lottery was chosen, is negative and highly significant for type-a lotteries and negative and insignificant for type-b lotteries. Chosen lotteries a were evaluated more positively, and, hence, received a lower rank in R_2, than comparable, but rejected lotteries a. As control for lottery specific features, we included the *Probability to win the high amount*, henceforth p, in lotteries a and b as a regressor. The corresponding coefficients were significant at the 5% level and negative. Lotteries with a higher p were evaluated more positively in R_2. In our set of lotteries for Experiment 1, a higher p translates into a lower amount to win (see Table 4 below). These lotteries thus represented low-risk, low-payout lotteries. Possible explanations include a tendency to hedge for low-risk lotteries or fatigue effects that could influence participants' propensity to take risks. All other controls were either insignificant or not robust across all model specifications. In particular, we controlled for the *High amount to win* in lotteries a and b (salience effects), the absolute value of the *Difference in expected values* between a and c and between b and d (the costs of violating FOSD), whether or not the associated decision in the choice task violated FOSD (dummy *Dominance*), whether or not the lottery was presented on the *Right*-hand side of the screen in the choice task, and the *Period* in which the choice was taken. For the random-effects approach we further controlled for the *High* treatment and several demographic factors like age, gender, previous participation in a statistics course, and following an economics-based study program.

A.2 Experiment 2

Table 3 reports generalized least squares regression results on the stated R_2-ranks for lotteries a and b in target pairs for Experiment 2. The interpretation is analogous to that of Table 2. The regression analysis confirms our findings as descried in the main text controlling for lottery-specific features, features of the choice environment, and participant-specific characteristics. The *Choice* dummy was negative and significant at the 5% level for type-b lotteries and insignificant for type-a lotteries. Lotteries b flagged as chosen received a better, and therefore lower rank, than rejected lotteries b. We again found that lottery characteristics, specifically the probability p to win the high amount as well as the high amount itself, influenced stated R_2-ranks independently of choice. A higher p and a higher amount to win were associated with higher R_2-ranks. These lotteries were thus evaluated more negatively. Our lottery-construction process in Experiment 2 did not follow a monotonic relationship between p and the high amount to win as it did for Experiment 1. This was necessary due to the different process for inducing FOSD relationships (see Table 5). Differences in the effect of lottery features on stated R_2-ranks across experiments could thus be attributed to the idiosyncratic

Table 2: Panel generalized least squares regressions on R_2-ranks for lotteries a and b in target pairs, Experiment 1. We present both random and fixed-effect approaches. Standard errors, reported in parentheses, are clustered at the participant level.

Dependent variable	Stated R_2-rank			Stated R_2-rank		
Sample	Lottery a, rank $R_1 = 3$			Lottery b, rank $R_1 = 4$		
	(1)	(2)	(3)	(4)	(5)	(6)
Choice	-0.187	-0.205	-0.194	-0.077	-0.059	-0.064
	(0.102)	(0.099)	(0.098)	(0.115)	(0.115)	(0.113)
Probability to win high amount		-1.139	-1.403		-0.873	-1.041
		(0.399)	(0.372)		(0.432)	(0.403)
High amount to win		-0.016	-0.005		0.005	0.006
		(0.041)	(0.034)		(0.031)	(0.027)
Difference in expected values		0.397	0.297		0.709	0.537
		(1.411)	(1.199)		(1.104)	(0.902)
Dominance		-0.921	-0.691		-0.107	-0.417
		(0.511)	(0.477)		(0.536)	(0.454)
Right		-0.192	-0.201		0.135	0.182
		(0.129)	(0.117)		(0.138)	(0.121)
Period		0.018	0.021		-0.022	-0.006
		(0.021)	(0.019)		(0.022)	(0.021)
High			-0.005			-0.197
			(0.169)			(0.175)
Constant	3.275	3.973	4.190	3.840	4.117	3.476
	(0.051)	(0.361)	(0.602)	(0.058)	(0.323)	(0.522)
Number of participants	140	140	140	140	140	140
Number of observations	560	560	560	560	560	560
Participant fixed-effects	Yes	Yes	No	Yes	Yes	No
Participant random-effects	No	No	Yes	No	No	Yes
Demographic controls	No	No	Yes	No	No	Yes
Cluster robust std. err.	Yes	Yes	Yes	Yes	Yes	Yes

differences between the two sets of lotteries used in the experiments. All other controls were either found insignificant or not robust across all model specifications.

B The lotteries

Tables 4 and 5 below present all 72 lotteries used in Experiment 1 and Experiment 2, respectively. The construction of the individual lotteries was guided by the following principles.

1. No (first-order) dominance relation obtains among any pair of lotteries within a given list.
2. Lotteries created during the choice task to induce dominance relations do not duplicate any of the existing lotteries from a given list.

Table 3: Panel generalized least squares regressions on R_2-ranks for lotteries a and b in target pairs, Experiment 2. We present both random and fixed-effect approaches. Standard errors, reported in parentheses, are clustered at the participant level.

Dependent variable	Stated R_2-rank			Stated R_2-rank		
Sample	Lottery a, rank $R_1 = 3$			Lottery b, rank $R_1 = 4$		
	(1)	(2)	(3)	(4)	(5)	(6)
Choice	0.041	0.046	0.067	-0.248	-0.242	-0.245
	(0.106)	(0.108)	(0.104)	(0.116)	(0.111)	(0.110)
Probability to win high amount		2.592	2.310		4.030	3.512
		(0.836)	(0.761)		(0.901)	(0.814)
High amount to win		0.466	0.448		0.576	0.473
		(0.095)	(0.090)		(0.089)	(0.081)
Difference in EV		1.415	0.607		-4.416	-5.721
		(3.950)	(3.522)		(4.440)	(3.946)
Dominance		-0.383	-0.243		-0.026	0.066
		(0.458)	(0.296)		(0.398)	(0.357)
Right		0.208	0.095		0.133	0.075
		(0.130)	(0.119)		(0.125)	(0.113)
Period		-0.029	-0.014		-0.016	-0.018
		(0.021)	(0.019)		(0.020)	(0.018)
High			-0.045			0.421
			(0.253)			(0.254)
Constant	3.137	-1.193	-0.202	3.878	-1.584	-1.228
	(0.052)	(0.885)	(0.873)	(0.059)	(0.845)	(0.819)
Number of participants	156	156	156	156	156	156
Number of observations	624	624	624	624	624	624
Subject fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Subject random-effects	No	No	No	No	No	No
Demographic controls	No	No	Yes	No	No	Yes
Cluster robust std. err.	Yes	Yes	Yes	Yes	Yes	Yes

3. All lotteries are non-degenerate, i.e. no certainty is involved.

4. The expected value of each lottery is bound between €3 and €5.

Point 1 aims to eliminate confounding factors in the ranking stages. The ranking of lotteries reflect subjects' preferences over lotteries involving trade-offs and are not deductible from first principles. Point 2 preserves the measurement validity for critical stimuli. No lottery was used both to elicit preferences and to induce choice patterns. Point 3 avoids pitfalls commonly identified in judgment and decision making (e.g. the certainty effect). The final point was motivated by our incentive scheme. Subjects were remunerated for three tasks. We expected the experiments to last about 1 hour. Subjects' expected earnings met the remuneration standards set out by the laboratory guidelines at the time ($\approx$ €12, excluding show-up fee).

Table 4: List of 36 lotteries used in Experiment 1. EV and Std. Dev. columns show the expected value and the standard deviation of the lottery.

p	Outcome 1	$(1-p)$	Outcome 2	EV	Std. Dev.
0.05	42	0.95	1	3.050	8.936
0.11	34	0.89	1	4.630	10.325
0.08	24	0.92	2	3.760	5.968
0.14	18	0.86	2	4.240	5.552
0.23	14	0.77	2	4.760	5.050
0.17	10	0.83	2.5	3.775	2.817
0.32	10	0.68	2	4.560	3.732
0.26	9	0.74	2.5	4.190	2.851
0.41	9	0.59	2	4.870	3.443
0.47	9	0.53	1	4.760	3.993
0.35	8.5	0.65	2.5	4.600	2.862
0.20	8	0.80	3	4.000	2.000
0.40	8	0.60	2.5	4.700	2.694
0.50	8	0.50	1.5	4.750	3.250
0.55	8	0.45	0.5	4.625	3.731
0.29	7.5	0.71	3	4.305	2.042
0.38	7	0.62	3	4.520	1.942
0.56	6.5	0.44	2.5	4.740	1.986
0.47	6.5	0.53	2	4.115	2.246
0.56	6.5	0.44	1.5	4.300	2.482
0.44	6	0.56	3	4.320	1.489
0.53	6	0.47	2.5	4.355	1.747
0.62	6	0.38	2	4.480	1.942
0.71	6	0.29	1	4.550	2.269
0.50	5.5	0.50	3.5	4.500	1.000
0.59	5.5	0.41	3	4.475	1.230
0.68	5.5	0.32	2.5	4.540	1.399
0.77	5.5	0.23	2	4.695	1.473
0.86	5.5	0.14	1	4.870	1.561
0.65	5	0.35	3.5	4.475	0.715
0.74	5	0.26	3	4.480	0.877
0.80	5	0.20	2.5	4.500	1.000
0.89	5	0.11	2	4.670	0.939
0.95	5	0.05	1	4.800	0.872
0.83	4.5	0.17	3.5	4.330	0.376
0.92	4.5	0.08	2.5	4.340	0.543

Table 5: List of 36 lotteries used in Experiment 2. EV and Std. Dev. columns show the expected value and the standard deviation of the lottery.

p	Outcome 1	$(1-p)$	Outcome 2	EV	Std. Dev.
0.06	42.15	0.94	1.75	4.174	9.594
0.08	32.85	0.92	1.85	4.330	8.410
0.10	24.75	0.90	2.15	4.410	6.780
0.12	16.65	0.88	2.25	3.978	4.679
0.14	14.55	0.86	2.35	4.058	4.233
0.16	9.45	0.84	2.45	3.570	2.566
0.18	9.35	0.82	2.55	3.774	2.612
0.20	9.25	0.80	2.65	3.970	2.640
0.22	8.75	0.78	2.75	4.070	2.485
0.24	8.65	0.76	2.85	4.242	2.477
0.26	8.55	0.74	3.15	4.554	2.369
0.28	7.45	0.72	3.25	4.426	1.886
0.30	7.35	0.70	3.35	4.550	1.833
0.32	7.25	0.68	3.45	4.666	1.773
0.34	6.85	0.66	3.55	4.672	1.563
0.36	6.65	0.64	3.65	4.730	1.440
0.38	6.45	0.62	3.75	4.776	1.311
0.61	6.35	0.39	1.75	4.556	2.244
0.40	6.25	0.60	3.85	4.810	1.176
0.63	6.25	0.37	1.85	4.622	2.124
0.65	6.15	0.35	2.15	4.750	1.908
0.67	5.85	0.33	2.25	4.662	1.693
0.69	5.75	0.31	2.35	4.696	1.572
0.71	5.65	0.29	2.45	4.722	1.452
0.73	5.55	0.27	2.55	4.740	1.332
0.75	5.45	0.25	2.65	4.750	1.212
0.77	5.35	0.23	2.75	4.752	1.094
0.79	5.25	0.21	2.85	4.746	0.978
0.81	5.15	0.19	3.15	4.770	0.785
0.83	4.85	0.17	3.25	4.578	0.601
0.85	4.75	0.15	3.35	4.540	0.500
0.87	4.65	0.13	3.45	4.494	0.404
0.89	4.55	0.11	3.55	4.440	0.313
0.91	4.45	0.09	3.65	4.378	0.229
0.93	4.35	0.07	3.75	4.308	0.153
0.95	4.25	0.05	3.85	4.230	0.087

C Experimental materials

We provide here a translated version of our experimental instructions. The original instructions were in German and are available upon request. We did not explain FOSD nor how choice pairs were constructed. This allowed us to use the same set of instructions across treatments and experiments. The instructions included a quiz about the pie-chart representation, which was checked by the experimenter present in a session. Participants were only allowed to continue with the experiment once all questions were answered correctly. This procedure ensured that all participants were able to read and understand the lotteries and their pie-chart representation. Participants also had the opportunity to gain familiarity with our computer interface in a practice round, which displayed letters from the alphabet instead of lotteries and asked participants to rank the letters in an alphabetically increasing order. Again, participants could only proceed to the actual experiment once they ranked the letters correctly. These measures lead us to conclude that all participants were capable of following our instructions and knew how to use the computer interface for ranking purposes.

The instructions included sample screen-shots that presented pie-chart representations of lotteries as well as a sample screen-shot from the decision screen in stage 1 and stage 3 of the experiment. We provide translated versions of the screen-shots used in the original German instructions in Figure 3. Additionally, we also provide sample screen-shots from our decision screens in Figure 4.

General Instructions

(After a general-purpose welcome text from the lab)

Here is a brief summary of today's protocol. The experiment consists of three independent decision-parts. Each one asks you to make several decisions. A small questionnaire will follow.

You will earn money for each decision part. How much money you will earn depends both on the decisions you make as well as on chance. The decision parts are independent from each other and the decisions taken in one part do not influence your payoff from any other part. We will add up your payoffs from the decision parts and you will also receive an additional €2.50 for showing-up punctually today. The total sum of payoffs will be paid out anonymously in cash after the experiment.

The next page contains further information about general procedures for today. All other relevant information will be distributed before a decision part starts.

General Instructions, Continued.

Your decisions today will be about lotteries. We will therefore explain to you in detail what is meant by a lottery and how it is represented on the computer screen.

A lottery consists of two possible EURO amounts which occur with certain probabilities. Lotteries will be represented as pie charts; see the example screen-shot below. The colored areas graphically represent the probabilities with which the monetary amounts are obtained. The lottery in the example below has two outcomes, so there are two colored areas.

Lottery example, continued

Consider the lottery from the example screen-shot *(here the left-hand-side lottery in Figure 3)*. The green area represents a 75% probability to win €10.00. The red area in the pie-chart represents a 25% probability to win €5.00. If this lottery is played out to determine your payoff, you would have the chance to win either of the two amounts. In 75 out of 100 random draws, you would win the €10.00, in 25 out of 100 random draws you would win the €5.00. Please notice that this is just an example. The actual lottery amounts and probabilities in the experiment will be different from the examples used here.

Please raise your hand if you have any further questions. If not, please answer the quiz on the next page.

Quiz lotteries

The picture below shows another example of a pie-chart representation of a lottery *(here the right-hand-side lottery in Figure 3)*. Please answer the following questions. Raise your hand when you are done and an assistant will come and check your answers.

1. What is the probability to win €3.00 for this lottery?
2. What is the probability to win €8.00 for this lottery?
3. Which amount can you win with a probability of 55% for this lottery?
4. Which amount can you win with a probability of 45% for this lottery?

Figure 3: Lotteries used in the instructions (translated).

Instructions, decision part 1

This part presents several lotteries on the screen simultaneously. For each displayed lottery simply imagine that you own this lottery and that it will be played out according to the probabilities and amounts specified in the pie-chart representation. Your task is to rank each lottery displayed on screen according to how much you would like to play it out in comparison to the other displayed lotteries.

In each decision round, the computer will display 6 different lotteries, see picture below *(see upper part of Figure 4)*. Please rank the lotteries in the following way.

- Please assign rank 1 first. Simply select the lottery among the displayed lotteries you would like to play out the most.
- Please assign rank 2 then. Simply select the lottery among the displayed lotteries you would like to play out the second-most.
- Please assign rank 3 then. Simply select the lottery among the displayed lotteries you would like to play out the third-most.

- Please assign rank 4 then. Simply select the lottery among the displayed lotteries you would like to play out the fourth-most.
- Please assign rank 5 then. Simply select the lottery among the displayed lotteries you would like to play out the fifth-most.
- Please assign rank 6 then.

To assign ranks simply click on the buttons below the lotteries. The assigned ranks will be displayed below the lotteries too.

If you are not satisfied with your rank assignment and wish to change it, simply click on reset. The rank assignment process will be restarted and you have to assign the ranks anew.

Once you have assigned rank 1 (the lottery you would like to play out the most) to rank 6 (the lottery you would like to play out the least), hit the 'Done' button to submit your ranks and to proceed to the next decision round.

There are no right or wrong answers in assigning ranks. When assigning ranks, simply ask yourself which is the lottery you would like to play out the most, second-most, etc. Please notice that the ranks influence your payoff for this decision part; see more details below.

In total, there will be 6 decisions rounds with 6 displayed lotteries each. The rounds are independent of one another. That is, you will be asked to rank 6 different lotteries according to your preferences 6 times. The next decision part will start as soon as you are done with this part.

Before you start ranking lotteries you will get the opportunity to gain familiarity with our computer interface in a practice round. The practice round is irrelevant for your actual remuneration today. Once you are done with the practice round, the first decision part will start.

Your payoff from decision part 1:

After you are done with ranking lotteries in all decision rounds, you will play out exactly one lottery. To determine which one it is, the computer will first randomly select one of the 6 decision rounds. Then the computer will randomly select two out of the 6 lotteries from this decision round. The computer will compare the two ranks of the two lotteries selected and you will play out the lottery you ranked as more desirable (that is, the lottery you would like to play out more from those two). The outcome that obtains from playing out this lottery is your payoff for decision part 1. You will receive all payoff-relevant information after you are done with all decision parts of today's experiment. Remember that although you will receive your payoff for this part at the end of the experiment, your decisions made in this part do not influence your payoff in any other part.

Please raise your hand if you have any further questions.

Instructions, decision part 2

This decision part of the experiment will present to you different lottery pairs sequentially. Your task is to choose one of the two displayed lotteries in each of the lottery pairs presented.

The lotteries will be displayed on-screen in the following way *(see lower part of Figure 4)*. One lottery will be shown at the left-hand side of the screen, the other lottery on the right-hand side of the screen. Below each lottery a button labeled 'This lottery' will be shown. To chose the left-hand side lottery simply click on the button below the left-hand side lottery. To chose the right-hand side lottery simply click on the button below this lottery. Please notice that the choices you make will influence your payoff from this decision part; see details below.

There are no right or wrong answers in making choices. Choices are just a way to for you to express which one of the two displayed lotteries you would like to play out more.

After you have indicated your choice, the next lottery pair will be presented. In total, there will be 10 lottery pairs and we will start with the next decisions part once you are done with indicating choice in all 10 lottery pairs.

Your payoff from decision part 2:

The computer will randomly select one of the 10 lottery pairs. For the selected pair, the computer will check which lottery you have chosen. This is the lottery you will play out. The outcome that obtains from playing out this lottery is your payoff for decision part 2. You will receive all payoff-relevant information after you are done with all decision parts of today's experiment. Remember that although you will receive your payoff for this part at the end of the experiment, your decisions made in this part do not influence your payoff in any other part.

Please raise your hand if you have any further questions.

Instructions, decision part 3

This part again presents several lotteries on screen simultaneously. The lotteries, your task, and the way your payoff is determined are the same as in decision part 1 of today's experiment. This is not a memory task. We want you take your decision as you feel right now. For your convenience, we will now re-state the instructions from part 1.

Instructions for part 1 repeated here.

Your payoff from decision part 3:

Instructions for part 1 repeated here.

Figure 4: Decision screens in the experiments. The top screen-shot presents the decision screen for the ranking tasks. The bottom screen-shot shows the decision screen for the choice task.